Introductory Essay, in Dr. Hooker's Flora of New Zealand: Vol. I.*

Dr. J. D. Hooker, the Botanist of the Antarctic Expedition under Capt. Sir James C. Ross, on his return to England—combining with his own extensive collections and observations all the accessible materials which have been accumulating in herbaria ever since the first voyage of Capt Cook—courageously assumed the task of preparing general floras of the three principal masses of southern land, in which his researches were made; viz., Antarctic America, New Zealand, and Tasmania. The first of these undertakings was accomplished several years since, in the publication of the *Flora Antarctica;* including Antarctic America with the Falkland Islands, the Campbell and Auckland Islands (properly pertaining to the New Zealand region), and the remote Kerguelen's Land. Some abstracts were given in this Journal at the time, from parts of the work possessing a general interest.

The recent completion of the fourth fasciculus, and first volume, of the second work, viz.: the *Flora Novæ Zelandiæ*, comprising all the Flowering plants of that group of islands, affords our author an opportunity to discuss, in an introductory essay, some topics of high interest to the philosophical naturalist; topics upon which his aptitude for such investigations, and the unparalleled opportunities of observation which he has enjoyed, and improved to the utmost, in almost every clime, must needs give no small weight to his opinions. Deeming as we do this essay to be an important and timely one, we propose to make a somewhat extended analysis of it, leaving out of view for the present the body of the work, as interesting to the systematic botanist alone.

In the Essay in question (occupying 39 quarto pages), Dr. Hooker gives, 1. The History of New Zealand Botany, and the probable limits of its flora. 2. An exposition of the views adopted in the descriptive part of the work, as to the affinities, limits, origin, variation, distribution, and dispersion of plants generally. 3. The illustration and development of these points by an analysis of the New Zealand Flora, and its relation to that of other countries.

The history of the Botany of New Zealand, from the visit of Sir Joseph Banks and Dr. Solander, during Capt. Cook's first voyage, in 1790, down to the present time, need not arrest our attention. The actual number of species inhabiting these islands is a matter which it would be interesting to know, even approximately. Dr. Hooker has brought together about 2,000 species in

* The Botany of the Antarctic Voyage of H. M. Discovery Ships Erebus and Terror, in the years 1839—1843, under the command of Capt. Sir James Clark Ross; by Joseph Dalton Hooker, M.D., etc. etc. II. Flora Novæ-Zelandiæ, Part I. Flowering Plants. London. Lovell Reeve, 1852–1853. pp. 312, 4to, tab. 70.

his Flora, including upwards of a hundred of the lower Cryptogamia of which the materials are in too imperfect a state for satisfactory determination. This is more than double the number comprised in the latest preceding catalogue, that of M. Raoul, "who in 1846 enumerates only 920 species; which may be reduced to 770, if the naturalized and erroneous species be eliminated." "In 1838, Mr. Cunningham gave 640 species, which should be reduced to 570: in 1832, M. Richard included 350 in his list: Forster's Prodromus has 154; and Banks' and Solander's collections amount to 426. This rapid increase of the flora, which has thus been quintupled in twenty years, is mainly due to the attention which has been devoted to the lower orders. This may be easily shown; for, whereas in all the earlier enumerations and collections the number of Flowering plants exceeds the Flowerless, in M. Raoul's catalogue they are equal, and in the present work the relative proportions are reversed; the Phænogamic plants being to the Cryptogamic as 1 to 1·6, i. e. about two to three." As to the probable ratio of the known materials to the whole flora, Dr. Hooker remarks that "the islands have been botanized upon by upwards of 35 individuals, whose specimens have (with a few unimportant exceptions) all passed under my eye. The flora of the Northern Island has been tolerably well examined, so far as its flowering plants are concerned; though there remains a good deal to be done on the west coast, especially in the neighborhood of Mount Egmont. Dr. Lyall alone has collected on the Southern Island, and on the west coast north of Dusky Bay. The Middle Island has been visited by few explorers; its north and east coasts alone having been botanized: the west, and the whole mountain range require a careful survey; and, considering how many Auckland and Campbell Islands' plants are still strangers to New Zealand, it cannot be doubted that much remains to be discovered there. Excepting from the above mentioned tracts, I do not expect much novelty among Flowering plants, for the following reasons: 1. There is a remarkable sameness in the flora throughout large tracts, (in which respect New Zealand contrasts remarkably with Tasmania); 2. Because out of the 730 flowering plants known, there are scarcely one hundred that have not been gathered by several individuals; 3. Because the collections I have lately received, though some of them are extensive, and from scarcely visited localities, yet contain little or no novelty. With Cryptogamia the case is widely different; and it is difficult to estimate the vast number, especially of Mosses, Hepaticæ, and Fungi, that will reward future explorers in what, as far as Flowering plants are concerned, are exhausted fields." From the data now possessed, and from a comparison of the same with the flora of better known countries, Dr. Hooker ventures the opinion that there are not more than 4000 species in

New Zealand, of which 1000 may be Flowering plants. "Compared with any other countries in the same latitude, this is a very scanty flora indeed, especially as regards Flowering plants; of which Britain contains, in about the same area, upward of 1,400 species; and in Tasmania, not yet well explored, and containing only one-third of the area, upwards of 1000 have already been discovered. In Cryptogamous plants, on the other hand, these islands are extremely rich, not only proportionately to the Phænogamic, but absolutely so. Great Britain, where the lower orders have been assidously studied for fifty years, contains about 50 Ferns, and Tasmania 64;" while Dr. Hooker's list of New Zealand Ferns (including Lycopodiaceæ), after reducing almost half as many nominal species to more varieties, contains upwards of 114 species. The same result would appear all the more strikingly on a comparison with any equivalent *continental* tract in the northern hemisphere. In all British America and Oregon only 62 species of Ferns and Lycopodiaceæ are recorded.

Dr. Hooker's second chapter, on the limits of species, their dispersion and variation, deals with matters of higher general interest. To bring the points in question fairly into view, he assumes four positions as heads of the subjects upon which he proceeds to discourse, namely:

"1. That all the individuals of a species (as I shall attempt to confine the term) have proceeded from one parent (or pair), and that they retain their distinctive (specific) characters.

2. That species vary more than is generally admitted to be the case.

3. That they are also much more widely distributed than is usually supposed.

4. That their distribution has been effected by natural causes; but that these are not necessarily the same as those to which they are now exposed."

The first of these propositions should have been divided into two:—inasmuch as the first clause is not only open to some very specious if not cogent objections which do not apply to the others, but is from its very nature incapable of being supported by the kind of evidence which may sustain the other propositions. The fact that we constantly see like individuals reproduced, and under favorable circumstances increased, from a parent stock, lays, indeed, a solid ground for the inference that this process has been going on from the beginning: but how things go on, and how they began, are two different questions; and it is seldom, if ever, that the facts and deductions which account for the former, can be made to throw much *direct* light on the latter. Why is it not antecedently just as probable that several or many individuals of each species of plant, as identically like each other as are the offspring to the parent now, were created at the beginning, as

that each began with a single pair? We would still maintain that the objective idea of species arises from this "perennial succession of like individuals (to use the phrase of Linnæus), sustaining to each other the relation of parent and progeny; and we think that the evidence on that side of the question strongly favors the inference, that plants, at least, have been distributed each species from a single and specific primordial area: but we know not what scientific evidence makes it needful to maintain the doctrine of the single creation of species in the restricted form of a single initial individual or pair. Were we even to go so far as explicitly to assume the likelihood, in certain cases, of the original creation of numerous individuals of a species, all alike in character, we could not be charged with infringing the rule that no more causes should be assumed than are requisite to ensure the result; since—to say nothing of the imminent risk of the premature destruction of a single individual—in many cases both of animals and vegetables, a considerable number would be required at the outset to fulfil the relations established between the individuals, either of the same or of different species. It would be more satisfactory, therefore, because less hypothetical and more within the reach of evidence, if the proposition were stated in the more general form; viz., that all the individuals of a species have proceeded from a common stock, assigned to a limited primordial area;—in which form its bearing upon the other propositions would not be altered. Indeed, Dr. Hooker expressly states, in the commencement of his remarks upon the first proposition, that he lays no stress upon that particular hypothesis, nor does he advance any arguments in support of it. The whole section is, in fact, mainly devoted to the illustration of the second clause, namely, that species retain their distinctive characters from age to age. As the section can hardly be abridged without injury, we extract a large portion of it. After stating various views that are maintained in respect to the origin or development of species, the author proceeds:

"Arguments in favor of these views are not wanting, derived both from the animal and vegetable kingdoms; the chief of which are drawn from a large class of well established facts, upon the bearings of which the most distinguished and candid naturalists are divided in opinion: such are—the great number of genera whose species have baffled all attempts at circumscription by fixed characters,—the facility with which breeds of certain plants and animals may be propagated, and the comparative certainty with which some few varieties are reproduced under favorable circumstances,—the great facility with which many plants hybridize, and the facts of hybrids having proved fertile,—the sudden appearance and unexplained cause of many varieties or sports,—and the difficulty of accounting for the existence of plants and animals in two or more localities, between which they cannot have been

transported by natural causes now in operation. These are all questions relating to the diffusion and variation of species, which will be discussed here and in the following section.

Arguments in favor of the single creation, and permanence of species, are all based upon general considerations of the phenomena of distribution. Comparative anatomy, which has thrown so great light upon this branch of study in the sister kingdom, has not done so much for plants; this arises from several causes: 1. The habits of allied plants do not differ so remarkably as those of animals, and there is consequently less modification of their functional organs. 2. The relation of these modifications to the habits and wants of the species, is in the animal kingdom directly appreciable, but in plants no such connection can be traced.* 3. The individual organs of support, respiration, and reproduction, are infinitely more variable and susceptible of change and even obliteration in plants, without affecting the life either of the individual or of the species.† The result of these facts is that we have the means in animals of appreciating the extent and value of differences, by combined observations upon structure and functions, upon habits and organization, which we have not in the vegetable kingdom, and which the phenomena of cultivation assure us do not exist to a degree that has, within the limits of our experience, proved available for throwing much light on the subject.

The arguments in favor of a permanence of specific characters in plants are:—

1. The fact that the amount of change produced by external causes does not warrant our assuming the contrary as a general law. Though there are many notorious cases in which cultivation and other causes produce changes of greater apparent value than specific characters generally possess, this happens in comparatively very few families, and only in such as are easily cultivated. In the whole range of the vegetable kingdom, it is difficult to produce a change of specific value, however much we may alter conditions; it is much more difficult to prevent an induced variety from reverting to its original state, though we perse-

* The structure of woods offers many illustrations of this; very closely allied plants (especially *Leguminosæ*) differing entirely in the nature, arrangement, and development of the vascular and cellular tissues of their trunks. Though to a great extent these differences accompany a habit of growth (as in the case of erect and scandent *Bauhinias*), there is nothing in the abnormally developed wood of the climbing *Bauhinia* that would lead a skilled physiologist ignorant of the fact to say that it was better adapted to a climbing than to an erect plant; the function is experimentally known to be indicated by the structure, but the structure is not seen to be adapted to the function. This is not so in the sister kingdom, for we confidently pronounce an animal to be a climber, because we see that its organs are adapted to the performance of that function; here the habit is not only indicated by the structure but the latter is explained by the function which it enables the animal to fulfill.

† To take an extreme case of this;—many plants are known, in a wild and cultivated state, which propagate abundantly by root or division, where they do not do so by seed. *Anacharis Alsinastrum* is a conspicuous example: it is a unisexual water-plant, of which one sex alone was introduced from North America into England, where it has within a few years so spread by division as to be a serious impediment to inland navigation. The Horse-radish is another example, it being, I believe, never known to seed or even to bear perfect flowers. A still more remarkable case has been pointed out to me by Mr. Brown, in the *Acorus Calamus*, a plant spread (not by cultivation) over the whole north temperate hemisphere, which bears hermaphrodite flowers, but very rarely seeds.

vere in supplying the original conditions; and it is most difficult of all to reproduce a variety with similar materials and processes.*

2. In tracing widely dispersed species, the permanence with which they retain their characters strikes the most ordinary observer; and this, whether we take such plants as have been dispersed without the aid of man (as *Sonchus oleraceus*, *Callitriche*, and *Montia*) through all latitudes from England to New Zealand; or such as have within modern times followed the migrations of man (as *Poa annua*, *Phalaris Canariensis*, Dock, Clover, *Alsine media*, *Capsella bursa-pastoris*, and a host of others); or such as man transports with him, whether such temperate climate plants as the cerealia, fruits, and flowers of the garden or field, or such tropical forms as *Convolvulus Batatas* and yams, which were introduced into New Zealand by its earliest inhabitants;—all these, in whatever climate to which we may follow them, retain the impress of their kind, unchanged save in a trifling degree.

3. With comparatively few exceptions, plants are confined within well-marked limits, which, though often very wide, are sometimes as much the reverse; while the instances are rare of sporadic species, as such are called which are found in small numbers in widely sundered localities. These facts seem incompatible on the one hand with the theory of species spreading from many centres, and on the other with their varying indefinitely; for were it otherwise, sporadic distribution would be the rule, insular floras would not necessarily be peculiar, and similar climates would have similar, if not identical species, which is not the case.

4. A multitude of allied species of plants grow close together without any interchange of specific character; and there are instances of exceedingly closely allied plants keeping company under many modifications of climate, soil, and elevation, yet never losing their distinctive marks.

5. The individuals that inhabit the circumference of the area occupied by a species, are not found passing into other species, but ceasing more or less abruptly; their limits may meet or overlap those of one or more very similar species; when the individuals associate, but do not amalgamate.

6. One negative argument in favor of distribution from one centre only, is, that taking the broadest view of the dispersion of species, we find that the more extensive families† are more or less widely distribu-

* I am quite aware that this argument will be met by many instances of change produced in our garden plants: but, after all, the skill of the gardener is successfully exerted in but few cases upon the whole: out of more than twenty thousand species cultivated at one time or another in the Royal Gardens of Kew, how few there are which do not come up not only true to their species, but even to the race or variety from which they spring; yet it would be difficult to suggest a more complete change than that from the Alps or Polar regions to Surrey, or from the free air of the tropics to the thoroughly artificial conditions of our hot-houses. Plants do not accommodate themselves to these changes: either they have passive powers of resisting their effects to a greater or less degree, or they succumb to them.

†This rule does not extend to the Natural Orders themselves. The *Compositæ*, whose facilities for dispersion are proverbial, are amongst the most local; and the same may be said of *Leguminosæ* and *Solaneæ*, whose seeds retain their vitality in a remarkable degree: a few of their species are remarkably cosmopolite, but the greater number have generally narrow ranges.

ted, very much in proportion to the facilities they present for dispersion. Thus the most minute-spored Cryptogams* are the most widely dispersed of all organized nature; plants that resist the influence of climate best, range furthest; water-plants are more cosmopolite than land-plants, and inhabitants of salt, more than those of fresh water: the more equable and uniform is the climate of a tract of land, the more uniformly and widely will its plants be distributed.

7. The species of the lowest orders are not only the most widely diffused, but their specific characters are not modified by the greatest changes of climate, however much their stature and luxuriance may vary. Fungi offer a remarkable instance of this: their microscopic spores are wafted in myriads through the air; the life of the individuals is often of very short duration, and many of them being as sensitive as insects to temperature and humidity, they are ephemeral in all senses; sometimes appearing only once in the same spot, and remaining but a few days, never to reappear within the observer's experience. The specific characters of many reside in the diameter, form, color, and arrangement of their most minute organs, whose analysis demands a refinement of microscopic skill; yet the most accomplished and profound botanist in this Natural Order (who has favored me with the descriptions of the New Zealand Fungi) fails to find the most trifling character by which to separate many New Zealand species from European.

8. The fact, now universally conceded by all intelligent horticulturists, that no plant has been acclimated in England within the experience of man, is a very suggestive one, though not conclusive; for it may be answered, that plants which cannot survive a sudden change, might a slow and progressive one. On the other hand, plants have powers of enduring change when self-propagated that they have not in our gardens; thus I find a great difference in the hardiness of individuals of several Himalayan plants,† depending upon the altitude at which they were gathered. In these cases the species is the same, and the parent individuals were not even varieties of one another, except so far as regards hardiness; in other words, the specific character remains unaltered in spite of the change of constitution, just as the climate of one part of the globe disagrees with the human race of another, and is even fatal to it.

Such are a few of the leading phenomena or facts that appear to me to give the greatest weight to the opinion that individuals of a species are all derived from one parent: for such arguments as the New Zealand Flora furnishes, I must refer my readers to the following chapter. I would again remind the student that the hasty adoption of any of these theories is not advisable: plants should be largely collected, and

* The fact (first communicated to me by the Rev. M. J. Berkeley) of the spores of Fungi having been found by Professor Ehrenberg mingled with the atmospheric dust that has fallen on ships far out at sea, is one of the most decisive proofs of this.

† Thus some of the seedling Pines whose parents grew at 12,000 feet appear hardy, whilst those of the same species from 10,000 are tender. The common scarlet *Rhododendron* of Nepal and the Northwest Himalaya is tender, but seedlings of the same species from Sikkim, whose parents grew at a greater elevation, have proved perfectly hardy.

studied both in the living and dried states, and the result of their dissection noted, without reference to any speculations, which are too apt to lead the inquirer away from the rigorous investigation of details, from which alone truth can be elicited. When however the opportunity or necessity arises for combining results, and presenting them in that systematic form which can alone render them available for the purposes of science, it becomes necessary for the generalizer to proceed upon some determinate principles."

The considerations here adduced bear partly in favor of the single creation of species of plants, partly on their permanence of specific characters from age to age; which are different questions, although closely connected. In respect to the first, we have the mere statement of the nature and kind of evidence that is available for the support of that doctrine: for its application to a particular flora, by which its amount and weight may be tested, we are referred to a subsequent portion of the essay. So, likewise, as to the permanence of species, our general conclusions must needs depend upon a knowledge of the limits within which the same species may vary; a topic which is discussed in the following section, mainly in view of its practical botanical applications. It were well that the considerations on which the doctrine of the single, as opposed to the double or multiple, origin of species rests, should be succinctly presented at one view, that their value may be estimated. For it is obvious that the very establishment of Dr. Hooker's second and third propositions may react powerfully upon the first. If species generally are much "more widely diffused than is usually supposed," then the theory of the double or multiple origin of species, as maintained by Schouw and others, becomes all the more likely, at least until adequate natural means of dispersion are clearly shown. And if, at the same time, "species vary in a state of nature more than is usually supposed," while what we term their specific characters are permanent, though the amount and value of these characters differs greatly in different species, then it may with the more probability be supposed that many of these differences were aboriginal. And these two inferences, taken together, would perhaps necessitate the conclusion, maintained we believe substantially by Prof. Agassiz, that species are ideal, as much so as genera, and even that they may have been represented from the beginning by as many individuals, and distributed over as wide an area, as at any subsequent time. A view which, by separating the idea of genetic connection from our conception of a species, seems to leave these no ground of objective reality to rest upon. We are reluctant to rest the very basis of natural history upon *a priori* conceptions, and therefore cling to the material view, that a species is not only "a primordial form" (the definition of the late Dr. Morton), but one represented in nature by the perennial succession of essentially like

individuals, which sustain to each other the relation of parent and offspring, and which, as we may actually follow them onward from generation to generation, so we may mentally trace them backward to a common and probably local stock of homogeneous individuals, if not to a single individual or pair. This furnishes a common standard to which our empirical determinations of species may be referred, and the discordant views of different describers be rendered commeasurable; as it reduces the differences to questions of the probable value of characters in each particular case. For the fact must not be overlooked, that the greater part of the species characterized in our systematic works are only provisional determinations—more or less probable conclusions from incomplete data.

Some valuable and timely remarks are to be found in the following extract:

"It is not surprising that two naturalists, taking opposite views of the value of characters, should so treat a variable genus that their conclusions as to the limits of its species should be wholly irreconcilable. Some naturalists consider every minute character, if only tolerably constant or even prevalent, as of specific value; they consider two or more doubtful species to be distinct till they are proved to be one; they limit the ranges of distribution, and regard plants from widely severed localities as almost necessarily distinct; they do not allow for the effects of local peculiarities in temperature, humidity, soil, or exposure, except they can absolutely trace the cause to the effect; and they hence attach great importance to habit, stature, color, hairiness, period of flowering, etc. These views, whether acknowledged or not, are practically carried out in many of the local floras of Europe, and by some of the most acute and observant botanists of the day; and it is difficult to over-estimate the amount of synonymy and confusion which they have introduced into the nomenclature of some of the commonest and most variable of plants. In such hands the New Zealand genera *Coprosma*, *Celmisia*, *Epilobium*, etc., may be indefinitely extended. The principles I have adopted are opposed to these: I have based my conclusions on this subject upon a very extensive examination of living plants in all latitudes, with my attention particularly directed to the influence of external causes, not only on the general phenomena of vegetation, but also upon individuals. Added to this, I have paid a great deal of attention to variable plants, both of tropical and temperate climates, and studied them in a living state, both wild and cultivated, and also in the herbarium. The result of my observations is, that differences of habit, color, hairiness, and outline of leaves, and minute characters drawn from other organs than those of reproduction, are generally fallacious as specific marks, being attributable to external causes, and easily obliterated under cultivation. It has hence been my plan to group the individuals of a genus which I assume after careful examination to contain many species whose limits I cannot define, so that the species shall have the same relative value as those have of allied genera whose specific characters are evident. In doing so I believe I have

followed the practice of every systematist of large experience and acknowledged judgment since the days of Linnæus, as Bentham, Brown, the De Candolles, Decaisne, Jussieu, Lindley, and the Richards; names which include not only the most learned systematists, but the most profound anatomists and physiologists. I am far from supposing that the same materials of a difficult group would receive precisely similar treatment at the hands of each of these eminent men; but their results would so closely approximate as to be in harmony with each other, and available for scientific purposes: with all, the tendency would be to regard dubious species as varieties, to take enlarged views of the range and variation of species, and to weigh characters not only *per se*, but with reference to those which prevail in the order to which the species under consideration belong.

In working up incomplete floras especially, I believe it to be of the utmost importance to adopt such a course, and to resist steadily the temptation to multiply names, for it is practically very difficult to expunge a species founded on an error of judgment or observation.* There is further an inherent tendency in every one occupied with specialities to exaggerate the value of his materials and labors, whence it happens, that botanists engaged exclusively upon local floras are at issue with those of more extended experience, the former considering as species what the latter call varieties, and what the latter suspect to be an introduced plant the former are prone to consider a native. There is much to be said on both sides of such questions: the local botanist looks closer, perceives sooner, and often appreciates better, inconspicuous organs and characters, which are overlooked or too hastily dismissed by the botanist occupied with those higher branches of the science, which demand a wider range of observation and broader views of specialities; and there is no doubt but that the truth can only be arrived at through their joint labors; for a good observer is one thing, and the knowledge and experience required to make use of facts for purposes of generalization, another; minute differences however, when long dwelt upon, become magnified and assume undue value, and the general botanist must always receive with distrust the conclusions deduced from a few species of a large genus, or from a few specimens of a widely distributed plant.

I have been led to dwell at length upon this point, because I feel sure the New Zealand student will at first find it difficult to agree with me in many cases, as for instance on so protean a Fern as *Lomaria procera*, whose varieties (to an inexperienced eye) are more dissimilar than are other species of the same genus. In this (and in many similar cases) he must bear in mind that I have examined many hundred specimens of the plant, gathered in all parts of the south temperate hemisphere, and have found, after a most laborious comparison, that I could not define its charcters with sufficient comprehensiveness from a

* The state of the British flora proves not only this, but further, that one such error leads to many more of the like kind: students are led to over-estimate inconstant characters, to take a narrow view of the importance and end of botany, and to throw away time upon profitless discussions about the difference between infinitely variable forms of plants, of whose identity really learned botanists have no doubt whatever.

study of its New Zealand phases alone, nor understand the latter without examining those of Australia, South Africa, and South America. The resident may find two varieties of this and of many other plants, retaining their distinctive characters within his own range of observation (for that varieties often do so, and for a very uncertain period, both when wild and also in gardens, is notorious), and he may perhaps have to travel far beyond his own island to find the link I have found, in the chain of forms that unites the most dissimilar states of *Lomaria procera;* but he can no more argue thence for the specific difference of these, than he can for a specific difference between the aboriginal of New Zealand and himself, because he may not find intermediate forms of his race on the spot. We do not know why varieties should in many cases thus retain their individuality over great areas, and lose them in others; but the fact that they do so proves that no deductions drawn from local observations on widely distributed plants can be considered conclusive. To the amateur these questions are perhaps of very trifling importance, but they are of great moment to the naturalist who regards accurately-defined floras as the means for investigating the great phenomena of vegetation; he has to seek truth amid errors of observation and judgment, and the resulting chaos of synonymy which has been accumulated by thoughtless aspirants to the questionable honor of being the first to name a species.*

There are many causes which render it extremely difficult to determine the limits of species, and in some genera the obstacles appear to increase, the more the materials for studying them multiply, and the more we follow our analysis of them into detail; hence the botanist is often led on to an indefinite multiplication of species (with increased difficulty of determining those already established), or to a reduction of all to a few, or to one variable species. My own impression is, that the progress of botany points to the conclusion that in many genera we must ultimately adopt much larger views of the variation of species than heretofore, and that the number of supposed kinds of plants is (as I shall indicate elsewhere) greatly over-estimated; if it be not so, we must either admit that species are not definable, or that there are hidden characters throughout all classes of the vegetable kingdom, of which the botanist has no cognizance, and towards the acquirement of which, if they are ever to be revealed, all efforts in the direction in which we have been advancing appear to be vain. Could systematists as a body be accused of carrying out their investigations in an unphilosophical manner or spirit, or without due attention to all the modes of testing the validity of characters, afforded by the study of living and dried plants, by direct observation, and by experiment, there might be hopes of such a revelation; but such hopes are inconsistent with the great advances that have been made in systematic botany, which, hav-

* The time however is happily past when it was considered an honor to be the namer of a plant; the botanist who has the true interests of science at heart, not only feels that the thrusting of an uncalled-for synonym into the nomenclature of science is an exposure of his own ignorance and deserves censure, but that a wider range of knowledge and a greater depth of study are required, to prove those dissimilar forms to be identical, which any superficial observer can separate by words and a name.

ing all tended to a more perfect knowledge of the affinities of plants, we are assured have been the effect of progress in the right direction."

Without doubt the preponderating tendency of the ablest and most experienced botanists of the present day is, to cancel nominal species, and, taking a more enlarged view of specific characters, to reduce slight or varying modifications to a common type; a point not yet reached by zoologists, though probably it will be hereafter. Dr. Hooker's tendency in this direction is evidently very decided; possibly too much so. But it must be allowed that, while the botanists who multiply species unduly are always those who work upon scanty materials, or whose personal observations are limited to a single district of country; on the other hand, those who have access to the largest collections, or who have themselves botanized over various parts of the world, are for the most part as strongly inclined in the opposite direction. Now Dr. Hooker possesses both these advantages in an eminent degree. Young as he still is, no living botanist has investigated on the spot so many and so widely separated floras, and few like him have had constant access to the largest and best determined herbaria in the world. The principal danger here arises from *l'embaras des richesses*. It is hardly possible that a vast series of apparently confluent forms should receive the detailed examination which the less privileged botanist may concentrate upon his fewer materials; and much is left to the quick, almost intuitive judgment, which is liable to error, indeed, but in which the true genius of a botanist is generally disclosed.

Perhaps there is no equally well known flora which compels the botanist to allow of such wide limits of variation in so large a proportion of its species, as that of New Zealand: at least so it would appear at first sight. Yet this character seems to be exhibited more or less by insular floras generally, in which a considerable number of the peculiar species are apt to be surprisingly polymorphous, as was remarked by Bory de St. Vincent half a century ago.

Hybridization has been supposed by many to be an important element in confusing and masking species. Botanists of one class are apt to refer to its agency the unwelcome appearance of a specimen which combines two nominal species, founded on inconstant characters. Another class of naturalists appeal to the repeated occurrence of fertile hybrids, to negative the inference, otherwise unavoidable, that the production between two individuals of inherently fertile offspring, is a good reason for pronouncing them identical in species. Any continued effects from hybridization in uncontrolled nature seem to be thoroughly guarded against in two ways; first by the constitutional debility, if not by the invariable sterility of the hybrid offspring, rendering it of transient duration; and secondly, by the fact that, when prolific at

all, they usually become so through fertilization by one or the other of the parents, when the offspring reverts to that specific type. Dr. Hooker justly remarks, that,

"As a general rule, the genera most easily hybridized in gardens are not those in which the species present the greatest difficulties. With regard to the facility with which hybrids are produced, the prevalent ideas on the subject are extremely erroneous. Gærtner, the most recent and careful experimenter, who appears to have pursued his inquiries in a truly philosophical spirit, says that 10,000 experiments upon 700 species produced only 250 true hybrids. It would have been most interesting had he added how many of them produced seeds, and how many of the latter were fertile, and for how many generations they were propagated. The most satisfactory proof we can adduce, of hybridization being powerless as an agent in producing species (however much it may combine them), are the facts that no hybrid has ever afforded a character foreign to that of its parents, and that hybrids are generally constitutionally weak, and almost invariably barren. Unisexual trees must offer many facilities for the natural production of hybrids, which nevertheless have never been proved to occur; nor are such trees more variable than hermaphrodite ones."

May we not even say that they are less variable *because* unisexual, since their progeny is most likely to be originated from the conjunction of different trees, and individual peculiarities must needs be thereby blended and obliterated. When we consider how often it must happen that the ovules of one tree in an oak or pine forest are fertilized by the pollen of another; when we take into view the great number of unisexual plants, and consider also how many apparently hermaphrodite blossoms (far more than is generally supposed) are really submonœcious or polygamous, we may find reason to suspect in this a more general provision of nature for limiting congenital or induced individual forms than would at first appear. That some such controlling or amalgamating agencies operate in nature may be inferred from a comparison of the general homogeneity of an indigenous species, over even a large area, with the ready development of marked varieties or races in the case of every cultivated plant which is multiplied from seed, and their perpetuation from generation to generation, which is almost always ensured merely by cultivating each stock by itself. For the general law of nature is, not merely that *species* are true to their characters, these characters being rightly apprehended, but that the *individual* progeny inherits and transmits the special peculiarities of the parent or parents. Hence, that an isolated race retains certain characteristics so long as kept separate is no proof that it constitutes a species. Many a variety of recent and known origin does this. But, on the other hand, whatever individuals, however distinguished by minor differences when separate, are found to blend into a fertile race when associated, must on sound principles be regarded as belonging to one

species. Perhaps if zoologists would contemplate the wide variations presented by many plants of indubitably one and the same species, and the still wider diversities of long cultivated races from an original stock, they would find more than one instructive parallel to the case of the longest-domesticated of all species, *man*.

Let it also be especially noted, that varieties are not always, not even generally, the result of external agencies, at least of such as we are able to detect. Certain varieties of plants are so originated: these are generally as transient as the cause that produces them, and under altered circumstances often disappear even during the life of the individual: the plant may outgrow them. But others arise, we know not how. It is past the wit of man to trace a connection between the diverse forms of the foliage, &c. of a polymorphous plant such as a *Coprosma*, *Metrosideros* or *Alseuosmia* in New Zealand, and any difference of circumstances attributable to station; still more so when these diversities occur side by side. Yet such are the varieties which ordinarily exhibit the greatest persistency, i. e. when kept from intermingling by mutual fecundation. What external circumstances can in the least account for the origin of the race of Dorking fowls, or Manx cats, or indeed of almost any of our domesticated races which were not produced by cross-breeding, that is by mingling the characters of two such races already in existence. Yet how certainly may we continue any of these races, under favorable circumstances, merely by maintaining the needful isolation; and how soon would they mingle and disappear if they were left to themselves. Is not this equally true of the human races?

From such facts, from the study of species remarkable for their polymorphous vegetation, as well as from the wide differences between certain domesticated races and their original stock,—differences not to have been expected on any known principles, and often far greater than those which are reckoned satisfactorily to mark the limits between other species,—the systematic botanist, at least, learns the lesson, that the amount of variation of which any one specific type is susceptible is only to be ascertained from observation and experience.

Upon the third proposition, that "Species are more widely diffused than is usually supposed," our author's statements are brief, but very decided and remarkable. He has already declared in the previous section "that it is by far the smaller half of the vegetable kingdom that is confined to narrow geographical or climatic areas, and that very few plants indeed are absolutely local; whilst the operations of the gardener and agriculturist prove that a vast proportion of the plants of the two temperate zones are capable of growing in any moderate climate." "I do not think," he continues, "that those who argue for narrow limits to the distribution and variation of species, can have considered

a garden in a philosophical spirit, or have weighed such facts as that there have been cultivated, within the last seventy years in the open air of England (at Kew), upwards of 20,000 species of plants from all quarters of the globe, and this within a space that, had it been left to nature, would not have contained 200 indigenous species. The fact that an overwhelming proportion of these have come up true to their parent, and have continued so under every possible disadvantage of transportation and transplantation, of altered seasons, and amount and distribution of temperature and humidity, of unsuitable soil and exposure, and of the multitude of errors which unavoidable ignorance of their natural locality and habit engenders: such appears to me the most forcible argument in favor of the power of plants to retain their original characters under altered circumstances."

The validity of the last inference is unquestionable and valuable. As long as the plants survived, they doubtless retained their characters. But the value of the general statement, as respects its bearing upon the natural diffusion of species, depends much on the answer to the question, how long did the majority of the species survive. We should like to know how large a proportion continued beyond the first two or three years, even with all the advantages of being looked after by the gardener.

But, returning to the general question, we remark, that there is little or no reason to expect a similarity between plants and animals in this respect. From the nature of the case we should suppose that the latter, at least that land-animals, would be much more local than plants.

We give to our readers the whole of the third section; as some of the statements, especially those that relate to the probable number of vegetable species known, are likely to excite some surprise.

"This is a point upon which my own views differ materially from those of many of my fellow botanists, and which, if borne out by facts, leads to a widely different estimate of the number and variety of the members of the vegetable kingdom than that which is at present entertained. As with the affinities and variation of species, so is it with their distribution: an extensive knowledge of the subject is only to be obtained by actual observation over large areas, and many of them, or by the study and comparison of the contents of many museums. It has been my singular good fortune to have visited many regions of the globe, and to have entered into some details upon the dispersion of living species, which has always been a favorite pursuit of mine. I have further had the advantage of collating my results with the largest and best-named botanical collections in the world, and have received a greater amount of assistance from my fellow naturalists than has fallen to the lot of most: facts which in ordinary cases are the result of long study and much consultation have been placed at my disposal rather than worked out by myself. A very extended examination of these materials has only tended to confirm the view which originated in my

personal experience, viz., that the estimate of the number of species known to botanists is a greatly exaggerated one,* and the prevalent ideas regarding their distribution no less contracted.

Many more plants are common to most countries than is supposed; I have found 60 New Zealand Flowering plants and 9 Ferns to be European ones, besides inhabiting various intermediate countries; and amongst the lower Orders we find a greatly increased proportion of species common to all countries: thus of Mosses alone 50 are found in New Zealand and Europe;† of Hepaticæ 13; of Algæ 45 are also natives of European seas; of Fungi nearly 60; and of Lichens 100.

So long ago as 1814 Mr. Brown‡ drew attention to the importance of such considerations, and gave a list of 150 European plants common to Australia. The identity of many of these has repeatedly been called in question, but almost invariably erroneously; added to which more modern collectors have greatly increased the list.

The too prevalent idea that the plants of newly discovered, isolated, or little visited localities must necessarily be new, has been a fertile source of the undue multiplication of species. There are very many cases of naturalists having been so impressed with this idea, that they have not thought it worth while to consult either books or herbaria before describing the plants from such spots. The New Zealand Flora presents several instances of this; two conspicuous ones occur in the genus *Oxalis;* one, *O. corniculata*, is amongst the most widely diffused and variable plants in the world; of its varieties no less than seven or eight species have been made, most of them supposed to be peculiar to New Zealand; not only is *O. corniculata* hence excluded from the flora, but, in the descriptions of these its varieties, no allusion is made to that plant.§ In the case of the other species the error is more ex-

* According to the loose estimate of compilers, 100,000 is the commonly received number of known plants: from a multiplicity of data I can come to no other conclusion but that half that number is much nearer the truth. This may well be conceived, when it is notorious that nineteen species have been made of the common Potato, and many more of *Solanum nigrum* alone. *Pteris aquilina* has given rise to numerous book species; *Vernonia cinerea* of India to fifteen at least. Many of the commonest European plants have several names in Europe, others in India, and still others in America, besides a host of garden names for themselves, their hybrids and varieties, all of which are catalogued as species in the ordinary works of reference whence such estimates are compiled.

† In fact the distribution of some Cryptogams is so wide, that I have visited a spot in a high southern latitude, nearly all whose plants are not only identical with those of Great Britain, but inhabit many intermediate temperate and tropical countries. Cockburn Island, in lat. 64° 12′ S. and long. 64° 49′ W., nearly fulfills this condition; I thereon collected nineteen plants, of which three-fourths are natives of England.

‡ Appendix to Flinders's Voyage, vol. ii, p. 592.

§ I have stated very confidently in the body of this work that eight of Cunningham's and Richard's species of this genus are all referable to one. This view will probably not meet the approbation of the local botanists, who will point to the constancy with which some of the states retain their characters under varied conditions. I value such facts very highly, and attach great weight to them, and did these varieties occur only in New Zealand I should perhaps have withheld so strong an opinion on the subject; but such is not the case. *O. corniculata* varies as much in numerous other parts of the world; and admitting, as every one must, that varieties are known to retain their characters with more or less constancy for certain periods, some other evidence is necessary to shake the opinion of the botanist who grounds his views on an examination of the plant from all quarters of the globe.

cusable, and may be still open to question;* it is that of *O. Magellanica*, originally discovered in Fuegia, and imperfectly described by Forster, whose very indifferent specimens of it are in the British Museum. When re-found in New Zealand it was described as new, and called *O. cataractæ*, and when found a third time in Tasmania, was called by still a third name, *O. lactea*. In this case a more important fact was smothered than that of the distribution of *O. corniculata*, namely, that of a very peculiar plant of the south temperate zone being common to these three widely sundered localities.

Many similar instances might be added, for there are several New Zealand plants (as *Pteris aquilina*) that have a different name in almost every country in the world: and, partly from changes in nomenclature, partly from the reduction of species, I have found myself obliged to quote 1500 names for the 720 New Zealand Flowering plants described; and I believe I might have doubled the number had my limits not obliged me to reduce the synonymy as much as possible; in many cases too much, I fear, for the requirements of working botanists in Europe."

One thing is clear, and important to be enforced: namely, that if determinations of species are to be of any value, especially in their bearing on general questions, they must rest solely upon observed characters of admitted value, irrespective of all theory. We pronounce such and such individuals, from a certain habitat, to belong to a distinct species, only because we find them possessed of certain adequate distinctive marks. If we at length ascertain that particular species are peculiar to particular stations or parts of the world, we have a sound and valuable deduction. But to assume that certain plants, or certain animals, from widely sundered localities belong to different species, notwithstanding their resemblance, until the contrary is proved, and even to announce this as a principle for general adoption, as has been done, is surely a gross instance of reasoning in a vicious circle.

We cannot venture to condense, and therefore reproduce the whole of § 4, viz.:

"*The distribution of species has been effected by natural causes, but these are not necessarily the same as those to which they are now exposed.*

Of all the branches of Botany there is none whose elucidation demands so much preparatory study, or so extensive an acquaintance with plants and their affinities, as that of their geographical distribution. Nothing is easier than to explain away all obscure phenomena of dispersion by several speculations on the origin of species, so plausible that the superficial naturalist may accept any of them; and to test their soundness demands a comprehensive knowledge of facts, which moreover run great risk of distortion in the hands of those who do not know

* As no identification is proved till all the organs of the plants to be compared have been studied, there is yet a possibility of these three species proving distinct, but I do not at all expect it; the only difference I can find is a greater obliquity and emargination of the petals of the New Zealand species, but that character varies so much both in this plant and in others of the genus that it loses all specific value.

the value of the evidence they afford. I have endeavored to enumerate the principal facts that appear to militate against the probability of the same species having originated in more places (or centres) than one; but in so doing I have only partially met the strongest argument of all in favor of a plurality of centres, viz., the difficulty of otherwise accounting for the presence in two widely sundered localities of rare local species, whose seeds cannot have been transported from one to the other by natural causes now in operation. To take an instance: how does it happen that *Edwardsia grandiflora* inhabits both New Zealand and South America? or *Oxalis Magellanica* both these localities and Tasmania? The idea of transportation by aerial or oceanic currents cannot be entertained, as the seeds of neither could stand exposure to the salt water, and they are too heavy to be borne in the air. Were these the only plants common to these widely-sundered localities, the possibility of some exceptional mode of transport might be admitted by those disinclined to receive the doctrine of double centres; but the elucidation of the New Zealand Flora has brought up many similar instances equally difficult to account for, and has developed innumerable collateral phenomena of equal importance, though not of so evident appreciation. These, which all bear upon the same point, may be arranged as follows:—

1. Seventy-seven plants are common to the three great south temperate masses of land, Tasmania, New Zealand, and South America.

2. Comparatively few of these are universally distributed species, the greater part being peculiar to the south temperate zone.

3. There are upwards of 100 genera, subgenera, or other well-marked groups of plants entirely or nearly confined to New Zealand, Australia, and extra-tropical South America. These are represented by one or more species in two or more of these countries, and they thus effect a botanical relationship or affinity between them all, which every botanist appreciates.

4. These three peculiarities are shared by all the islands in the south temperate zone (including even Tristan d'Acunha, though placed so close to Africa), between which islands the transportation of seeds is even more unlikely than between the larger masses of land.

5. The plants of the Antarctic islands which are equally natives of New Zealand, Tasmania, and Australia, are almost invariably found only on the lofty mountains of these countries.

Now as not only individual species, but groups of these, whether orders, genera, or their subdivisions, are to a great degree distributed within certain limits or areas, it follows that the flora of every island or archipelago presents peculiarities of its own. Though an insular climate may favor the relative abundance of individuals, and even species of certain Natural Orders, there is nothing in the climate, or in any other attribute of insularity, which indicates the nature of the peculiarity of endemic species. The islands of each ocean contain certain botanically allied forms in common, which are more or less abundant in them, and rarely or never found on the neighboring continents; thus there are curious genera peculiar to the North Atlantic islands, others to the North Pacific islands, others to those of the South Pacific and others again to the Malayan Archipelago; just as there are still

others peculiar to the Antarctic islands, and many to New Zealand, Fuegia, and Tasmania.

Each group of islands hence forms a botanical region, more or less definable by its plants as well as by its oceanic boundaries; precisely as a continuous area like Australia or South Africa does. There is however this difference, that whereas the Natural Orders that give a botanical character to a continuous area of a continent or to a large island (as the *Proteaceæ* in South Africa or in New Holland, and *Coprosma* in New Zealand) are numerous in species and often uniformly spread,—in clusters of small islands, distant from continents, they are few in species, and the individuals are scattered, appearing like the vestiges of a flora which belonged to another epoch, and which is passing away. This is perhaps a fanciful idea, but one which I believe to contain the germ of truth; for no Botanist can reflect upon the destruction of peculiar species on small islands (such as is now going on in St. Helena amongst others), without feeling that, as each disappears, a gap remains, which may never be botanically refilled; that not only are those links breaking by which he connects the present flora with the past, but also those by which he binds the different members of the vegetable kingdom one to another. It is not true in every sense that all existing nature appears to the naturalist as an harmonious whole; each species combines by its own peculiarities two or more others more closely, and reveals their affinities more clearly, than any other does; just as the flora of an intermediate spot of land connects those of two adjacent areas better than any other locality does. It is often by one or a very few species that two large Natural Orders are seen to be related; just as by a few Chilian plants the whole flora of New Zealand is connected with that of South America. The destruction of a species must hence create an hiatus in our systems, and I believe that it is mainly through such losses that natural orders, genera, and species become isolated, that is, peculiar, in a naturalist's eyes.

To return to the distribution of existing species, I cannot think that those who, arguing for unlimited powers of migration in plants, think existing means ample for ubiquitous dispersion, sufficiently appreciate the difficulties in the way of the necessary transport. During my voyages amongst the Antarctic islands, I was led, by the constant recurrence of familiar plants in the most inaccessible spots, to reflect much on the subject of their possible transport; and the conviction was soon forced upon me, that, putting aside the almost insuperable obstacles to trans-oceanic migration between such islands as Fuegia and Kerguelen's Land, for instance (which have plants in common, not found elsewhere), there were such peculiarities in the plants so circumstanced, as rendered many of them the least likely of all to have availed themselves of what possible chances of transport there may have been. As species they were either not so abundant in individuals, or not prolific enough to have been the first to offer themselves for chance transport, or their seeds presented no facilities for migration,* or were singularly perishable from feeble vitality, soft or brittle integuments, the presence of oil that soon became rancid, or from having a fleshy albumen that

* Thus of the *Compositæ*, common to Lord Auckland's Group, Fuegia, and Kerguelen's Land, none have any pappus (or seed-down) at all! Of the many species *with* pappus, none are common to two of these islands!

quickly decayed.* Added to the fact that of all the plants in the respective floras of the Antarctic islands those common to any two of them were the most unlikely of all to emigrate, and that there were plenty of species possessing unusual facilities, which had not availed themselves of them, there was another important point, namely, the little chance there was of the seeds growing at all, after transport. Though thousands of seeds are annually shed in those bleak regions, few indeed vegetate, and of these fewer still arrive at maturity. There is no annual plant in Kerguelen's Land, and seedlings are extremely rare there; the seeds, if not eaten by birds, either rot on the ground or are washed away; and the conclusion is evident, that if such mortality attends them in their own island, the chances must be small indeed for a solitary individual, after being transported perhaps thousands of miles, to some spot where the available soil is pre-occupied.

Beyond the bare fact of the difficulty of accounting by any other means for the presence of the same species in two of the islands, there appeared nothing in the botany of the Antarctic regions to support or even to favor the assumption of the double creation, and I hence dismissed it as a mere speculation which, till it gained some support on philosophical principles, could only be regarded as shelving a difficulty; whilst the unstable doctrine, that would account for the creation of each species on each island by progressive development on the spot, was contradicted by every fact.

It was with these conclusions before me, that I was led to speculate on the possibility of the plants of the Southern Ocean being the remains of a flora that had once spread over a larger and more continuous tract of land than now exists in that ocean; and that the peculiar Antarctic genera and species may be the vestiges of a flora characterized by the predominance of plants which are now scattered throughout the southern islands. An allusion to these speculations was made in the 'Flora Antarctica' (pp. 210 and 368), where some circumstances connected with the distribution of the Antarctic islands were dwelt upon, and their resemblance to the summits of a submerged mountain chain was pointed out; but beyond the facts that the general features of the flora favored such a view, that the difficulties in the way of transport appeared to admit of no other solution, and that there are no limits assignable to the age of the species that would make their creation posterior to such a series of geological changes as should remove the intervening land, there was nothing in the shape of evidence by which my speculation could be supported. I am indebted to the invaluable labors of Lyell and Darwin,† for the facts that could alone have given

* Of the seeds sent to England from the Antarctic regions, or transported by myself between the several islands, almost all perished during transmission.

† See Darwin's 'Journal of a Naturalist,' and 'Essays on Volcanic Islands and Coral Islands.' The proofs of the coasts of Chili and Patagonia having been raised continuously, for several hundred miles, to elevations varying between 400 and 1300 feet, since the period of the creation of existing shells, will be found in the first-named of these admirable works, which should be in the hands of every New Zealand Naturalist, if only from its containing important observations on his own islands. The fact of this accomplished Naturalist and Geologist having preceded me in the investigation of the Natural History of the Southern Ocean, has materially influenced and greatly furthered my progress; and I feel it the more necessary to

countenance to such an hypothesis; the one showing that the necessary time and elevations and depressions of land need not be denied; and the other, that such risings and sinkings are in active progress over large portions of the continents and islands of the southern hemisphere. It is to the works of Lyell* that I must refer for all the necessary data as to the influence of climate in directing the migration of plants and animals, and for the evidence of the changes of climate being dependent on geological change. In the 'Principles of Geology' these laws are proved to be of universal application, and amply illustrated by their being applied to the elucidation of difficult problems in geographical distribution. It follows from what is there shown, that a change in the relative positions of sea and land has occurred to such an extent, since the creation of still existing species, that we have no right to assume that the plants and animals of two given areas, however isolated by ocean, may not have migrated over pre-existing land between them. This was illustrated by an examination of the natural history of Sicily (where land-shells, still existing in Italy, and which could not have crossed the Straits of Messina, are found imbedded on the flanks of Etna high above the sea-level), regarding which Sir Charles Lyell states that most of the plants and animals of that island are older than the mountains, plains, and rivers they now inhabit.†

It was reserved for Professor Edward Forbes, one of the most accomplished naturalists of his day, to extend and enlarge these views, and to illustrate by their means the natural history of an extensive area; which he did by applying a profound knowledge of geology and natural history to the materials he had collected during his arduous surveys of many of the shores of Europe and the Mediterranean. The result has been the enunciation of a theory, from which it follows that the greater part, if not all, of the animals and plants of the British Islands have immigrated at different periods, under very different climatic conditions; and that all have survived immense changes in the

mention this here, because Mr. Darwin not only directed my earliest studies in the subjects of the distribution and variation of species, but has discussed with me all the arguments, and drawn my attention to many of the facts which I have endeavored to illustrate in this Essay. I know of no other way in which I can acknowledge the extent of my obligation to him, than by adding that I should never have taken up the subject in its present form, but for the advantages I have derived from his friendship and encouragement.

* To Sir Charles Lyell's works, indeed, I am indebted for the enunciation of those principles that are essential to the progress of every naturalist and geologist; those, I mean, that affect the creation and extinction, dispersion and subsequent isolation of organic beings; and though botanists still differ in opinion as to the views he entertains on the most speculative of subjects (the origin and permanence of species), there is, I think, but one as to the soundness and originality of his observations on all that relates to the strict dependence of organic beings on physical conditions in the state of the earth's surface. I feel that I cannot over-estimate the labors of this great philosopher, when I reflect that without them the science of geographical distribution would have been with me little beyond a tabulation of important facts; and that I am indebted to them, not only for having given a direction to my studies in this department, but for an example of admirable reasoning on the facts he has collected regarding the distribution of plants and animals. I have no hesitation in recommending the 'Principles of Geology' to the New Zealand student of Nature, as the most important work he can study.

† See the Principles of Geology, ed. 9. p. 702, and Address to the Geological Society of London by the President (Leonard Horner, Esq.), in 1847, p. 66.

configuration of the land and seas of Northern Europe. The arguments which support this theory are based upon evidence derived from Zoology and Geology,* and they receive additional weight from the fact that the distribution of British plants is in accordance with its principal features.†

The geographical distribution of British plants has been the subject of the most rigorous investigation by one of our ablest British botanists, Mr. H. C. Watson, who first drew attention to the various botanical elements of which the flora is composed, and grouped the species into botanical provinces. These provinces were intended for 'showing the areas of plants, as facts in nature independent of all theoretical explanations and reasons.' (Cybele Britannica, vol. i, p. 18). An inspection of them shows the relations borne by the plants of England to those of certain parts of Europe and of the Arctic regions; and Professor Forbes, applying a modification of these botanical provinces to the illustration of his views of the original introduction of plants into the British Islands, proceeds to show that their migration took place at different periods, contemporary of course with the connection by land of each botanical region of Britain with that part of the continent which presents a similar association of plants.

To extend a theoretical application of these views to the New Zealand Flora, it is necessary to assume that there was at one time a land communication by which the Chilian plants were interchanged; that at the same or another epoch the Australian, at a third the Antarctic, and at a fourth the Pacific floras were added to the assemblage. It is not necessary to suppose that for this interchange there was a continuous connection between any two of these localities, for an intermediate land, peopled with some or all of the plants common to both, may have existed between New Zealand and Chili when neither of these countries was as yet above water.‡ To account, however, for the Antarctic

* For the contents of the Essay itself, I must refer to the Records of the Geological Survey of Great Britain, vol. i. p. 336. This is the most original and able essay that has ever appeared on this subject, and though I cannot subscribe to all its botanical details, I consider that the mode of reasoning adopted is sound, and of universal application. What I dissent from most strongly is, the origin of the gulf-weed, the peopling of Scotch mountains by iceberg transport of seeds, and the too great stress laid upon the west Irish flora, whose peculiarities appear to me to be considerably over-estimated.

† It may be well to state to the New Zealand student, that there are no reasons to suppose that Botany can ever be expected to give that direct proof of plants having survived geological changes of climate, sea, and land, which animals do; the cause is evident, for the bones of quadrupeds, shells of mollusca, and hard parts of many animals, afford an abundant means of specific identification, and such are preserved when the animals perish. In plants the case is widely different: their perishable organs of reproduction, which alone are available for systematic purposes, are seldom imbedded, even when other parts of the plants are.

‡ This disappearance of old land, and the migration of its flora and fauna to new, may be illustrated to a certain extent by the delta of any New Zealand river. A mud-bank on one shore, covered with mangroves, advances across the channel, the mangroves growing on the new land as it forms. The current changes, and the end of the bank (with its mangroves) is cut off, and becomes an island: another change of the river channel fills up that between the islet and the opposite shore, to which it hence becomes a peninsula, peopled by mangroves, whose parents grew on the opposite bank. Here, be it remarked, no subsidence is required, such as must have operated in the assumed isolation of New Zealand.

plants on the lofty mountains, a new set of influences is demanded; no land connection between these islands and New Zealand could have effected this, for the climate of the intermediate area must necessarily have prevented it. But changes of relation between sea and land induce changes of climate, and the presence of a large continent connecting the Antarctic islands would, under certain circumstances render New Zealand as cold as Britain was during the glacial epoch. Sir C. Lyell first demonstrated this, and showed what such conditions should be; and by consulting the 'Principles of Geology,' my reader will understand how such a climate would reign in the latitude of New Zealand, as that its flora should consist of what are now Antarctic forms of vegetation. The retirement of the plants to the summit of the New Zealand mountains,* would be the necessary consequence of the amelioration of climate that followed the isolation of New Zealand, and the replacement of the Antarctic continent by the present ocean.

The climate throughout the south temperate zone is so equable, and the isothermal lines are so parallel to those of latitude, that it is not easy for the New Zealand naturalist to realize the altered circumstances that would render the plains of his island suitable for the growth of plants that now inhabit its mountains only;† but if he glance at the map of the isothermal lines of the northern hemisphere, he will see how varied are the climates of regions in the same latitude; that London, with a mean temperature of 51°, is in the same latitude as Hudson's Bay, where the mean temperature is 30°, and the soil ever frozen: and he will further be able to understand by a little reflection, how a change in the relative positions of sea and land would, by isolating Labrador, raise its temperature 10°–15°, causing the destruction of all the native plants that did not retire to its mountain-tops, and favoring the immigration of the species of a more genial climate.

The first inference from such an hypothesis is that the Alpine plants of New Zealand, having survived the greatest changes, are its most ancient colonists; and it is a most important one in many respects, but especially when considered with reference to the mountain floras of the

* With regard to the British mountains, Professor Forbes imagines that they were islets in the glacial ocean, and received their plants by transportation of seeds with soil, on ice from the Arctic regions. This appears to me to want support, and there is much in the distribution of Arctic plants especially, wholly opposed to the idea of ice transport being an active agent in dispersion. A lowering of 10° of mean temperature would render the greater part of Britain suitable to the growth of Arctic plants; it would give it the climate of Labrador, situated in the same latitude on the opposite side of the Atlantic. Britain is the warmest spot in its latitude, and a very slight geological change would lower its mean temperature many degrees.

† The New Zealand naturalist has probably a very simple means of determining for himself whether his island has been subject to a geologically recent amelioration of climate; to do which, let him examine the fiord-like bays of the west coast of the Middle Island, for evidence of the glaciers which there exist in the mountains having formerly descended lower than they now do. Glaciers to this day descend to the level of the sea in South Chili, at the latitude of Dusky Bay; and if they have done so in the latter locality, they will have left memorials, in the shape of boulders, moraines, and scratched and polished rocks.

Pacific and southern hemisphere generally. These may be classed under three heads:*—

1. Those that contain identical or representative species of the Antarctic Flora, and none that are peculiarly Arctic; as the Tasmanian and New Zealand Alps.†

2. Those that contain, besides these, peculiarities of the Northern and Arctic Floras;‡ as the South American Alps.

3. Those that contain the peculiarities of neither; as the mountains of South Africa and the Pacific Islands.

We thus observe that the want of an Arctic or Antarctic Flora at all in the Pacific islands, and the presence of an Arctic one in the American Alps, are the prominent features; and I shall confine my remarks upon these to the fact that, with regard to the isolated islands of the Pacific, they are situated in too warm a latitude to have had their temperature cooled by changes in the relative position of land and ocean, so as to have harbored an Antarctic vegetation. With regard to the South American Alps, there is direct land communication along the Andes from Arctic to Antarctic regions; by which not only may the strictly Arctic genera and species have migrated to Cape Horn, but by which many Antarctic ones may have advanced northward to the equator.§

There is still another point in connection with the subject of the relative antiquity of plants, and in adducing it I must again refer to the 'Principles of Geology,' where it is said, 'As a general rule, species common to many distant provinces, or those now found to inhabit many distant parts of the globe, are to be regarded as the most ancient . . . . their wide diffusion shows that they have had a long time to spread themselves, and have been able to survive many important changes in Physical Geography.'‖ If this be true, it follows that, consistently with the theory of the antiquity of the Alpine flora of New Zealand, we should find amongst the plants common to New Zealand and the Antarctic islands, some of the most cosmopolitan; and we do so in *Montia*

* I need scarcely remind my reader that in thus sketching the characteristics of these Alpine floras, I make no allusion to exceptions that do not alter the main features. I am far from asserting that there are no peculiar Arctic or Antarctic forms in the Pacific Islands, nor any peculiarly Arctic ones in Tasmania and New Zealand: but if, on the one hand, future discoveries of such shall weaken the points of difference between these three mountain regions, on the other they might be very much strengthened by adducing the number of Arctic species common to the South American Alps, but not found in the others.

† These Antarctic forms are very numerous; familiar ones are *Acæna, Drapetes, Donatia, Gunnera, Oreomyrrhis, Lagenophora, Forstera, Ourisia, Fagus, Callixene, Astelia, Gaimardia, Alepyrum, Oreobolus, Carpha, Uncinia.*

‡ *Berberis, Sisymbrium, Thlaspi, Arabis, Draba, Saxifraga, Lychnis, Cerastium, Fragaria, Lathyrus, Vicia, Hippuris, Chrysosplenium, Ribes, Saxifraga, Valeriana, Aster, Hieracium, Stachys, Primula, Anagallis, Pinguicula, Statice, Empetrum, Phleum, Elymus, Hordeum.*

§ Why these Antarctic forms have not extended into North America, as the Arctic ones have into South America, is a curious problem, and the only hypothesis that suggests itself is derived from the fact that though the Panama Andes are not now sufficiently lofty for the transit of either, there is nothing to contradict the supposition that they may have had sufficient altitude at a former period, and that one which preceded the advance of the Antarctic species to so high a northern latitude.

‖ Principles of Geology, ed. 9, p. 702.

fontana, *Callitriche verna*, *Cardamine hirsuta*, *Epilobium tetragonum*, and many others.

On the other hand, it must be recollected that there are other causes besides antiquity and facility for migration, that determine the distribution of plants; these are their power, mentioned above, of invading and effecting a settlement in a country preoccupied with its own species, and their adaptability to various climates: with regard to the first of these points, it is of more importance than is generally assumed, and I have alluded to its effects under *Sonchus*, in the body of this work. As regards climates, the plants mentioned above seem wonderfully indifferent to its effects.*

Again, even though we may safely pronounce most species of ubiquitous plants to have outlived many geological changes, we may not reverse the position, and assume local species to be amongst the most recently created; for whether (as has been conjectured) species, like individuals, die out in the course of time, following some inscrutable law whose operations we have not yet traced, or whether (as in some instances we know to be the case) they are destroyed by natural causes (geological or others), they must in either case become scarce and local while they are in process of disappearance.

In the above speculative review of some of the causes which appear to affect the life and range of species in the vegetable kingdom, I have not touched upon one point, namely, that which concerns the original introduction of existing species of plants upon the earth. I have assumed that they have existed for ages in the forms they now retain, that assumption agreeing, in my opinion, with the facts elicited by a survey of all the phenomena they present, and, according to the most eminent zoologists, with those laws that govern animal life also; but there is nothing in what is assumed above, in favor of the antiquity of species and their wide distribution, that is inconsistent with any theory of their origin that the speculator may adopt. My object has not so much been to ascertain what may, or may not, have been the original condition of species, as to show that, granting more scope for variation than is generally allowed, still there are no unassailable grounds for concluding that they now vary so as to obliterate specific character; in other words, I have endeavored to show that they are, for all practical purposes of progress in botanical science, to be regarded as permanently distinct creations, which have survived great geological changes, and which will either die out, or be destroyed, with their distinctive marks unchanged. We have direct evidence of the impoverishment of the flora of the globe, in the extinction of many most peculiar insular species within the last century; but whether the balance of nature is kept up by the consequent increase of the remainder in individuals, or by the sudden creation of new ones, does not appear, nor have we any means of knowing: if the expression of an opinion be insisted on,

* Mr. Watson (Cybele Britannica) gives the range of *Callitriche* in Britain alone as including mean temperatures of 40° to 52°, and as ascending from the level of the sea to nearly 2000 feet in the East Highlands of Scotland. *Montia*, according to the same authority, enjoys a range of 36° to 52°, and ascends to 3300 feet; *Epilobium*, a temperature of 40° to 51°, and ascends to 2000 feet; *Cardamine*, a temperature of 37° to 52°, and ascends to 3000 feet.

I should be induced to follow the example of an eminent astronomer, who, when the question was put to him, as to whether the planets are inhabited, replied that the earth was so, and left his querist to argue from analogy. So with regard to species, we know that they perish suddenly or gradually, without varying into other forms to take their place as species, from which established premiss the speculator may draw his own conclusions. * * * * *

To those who may accuse me of giving way to hasty generalization or loose speculation on the antiquity and dispersion of plants over parts of the Southern Hemisphere, I may answer, that no speculation is idle or fruitless, that is not opposed to truth or to probability, and which, whilst it co-ordinates a body of well established facts, does so without violence to nature, and with a due regard to the possible results of future discoveries. I may add, that after twelve years' devotion to the laborious accumulation and arrangement of facts in the field and closet, untrammelled by any theories to combat or vindicate, I have thought that I might bring forward the conclusions to which my studies have led me, with less chance of incurring such a reproach, than those would, who with far better abilities and judgment, have not had my experience and opportunities."

The hypothesis that these insular floras are the remains of larger floras greatly reduced or verging to extinction, will be found remarkably applicable, if we mistake not to the case of the Sandwich Islands; many of the characteristic species of which appear to be represented by comparatively few individuals, as if waning to extinction along with the Hawaian race.

The general idea here so ably set forth by Dr. Hooker, in respect to the Antarctic and South Pacific floras, is a very important one, and worthy of the most extended and critical examination. Its establishment, moreover, by satisfactory evidence, would destroy one of the strongest grounds upon which the doctrine of the multiple origin of species (at least in the form maintained by Schouw) is supported. To complete the view, and fairly to exhibit the grounds upon which these theoretical conclusions are based, we should embrace in our extracts a large part of the remaining chapter; on the physiognomy and affinities of the New Zealand Flora, and on the variation of New Zealand species. But the necessary limits of our article forbid. A few facts regarding the more striking peculiarities of the New Zealand flora may be collected. The large proportion, both relatively and absolutely, of the Cryptogamia, and especially of Ferns, has already been noted. "A paucity of Grasses, and absence of Leguminosæ, and abundance of bushes and Ferns, and a want of annual plants, are the prevalent features of the open country; whilst the forests abound in Cryptogamia, in Phænogamic plants with obscure and green flowers, and very often of obscure and little known natural orders."

The number of natural orders of the Phænogamic plants is remarkably large in proportion to the genera, even for an insular flora; being 92 to 282, or about one to three: while the genera

are to the species as 282 to 730, each genus having on the average only 2½ species: so that the species average but eight for each order. This makes it one of the most difficult floras in the world for a beginner, who must know a natural order for every eight species. How recondite, vague, and unsatisfactory the natural system of Botany must appear to the New Zealand student!

Of the largest natural orders, as respects the number of species, the individuals are often so few that none of them form prominent features in the landscape. The *Coniferæ* prove to be the most prevalent family; but the majority of their species are not social but grow intermixed with the trees, so as to give no character to the landscape;—a case just opposite to what occurs in the northern hemisphere, at least in North America, where the species are few, but mostly social, and existing in a vast number of individuals, which often occupy considerable tracts almost exclusively, and thus strikingly impress their features upon the landscape.

The number of kinds of trees is very large in proportion to the herbaceous plants; as there are 113 Phænogamous trees, including shrubs above twenty feet high, or one-sixth of the flora, while in England there are not more than 35 native trees, in a much larger flora.

A remarkably large proportion of the Phænogamous flora of New Zealand consists of absolutely peculiar plants: of these there are 26 genera and 507 species; or more than two-thirds of the whole. The greater part of these are Exogens. Of the remaining third, consisting of plants common to New Zealand and other countries:

"193 species, or nearly one-fourth of the whole, are Australian.
89 species, or nearly one-eighth of the whole, are South American.
77 species, or nearly one-tenth of the whole, are common to both the above.
60 species, or nearly one-twelvth of the whole, are European.
50 species, or nearly one-sixteenth of the whole, are Antarctic Islands, Fuegian, &c."

These several elements in the New Zealand flora, whether as represented by identical or cognate species, are in turn subjected to a critical analysis. The following extract, respecting the Australian element, is interesting from its direct bearing on the question of transport by water.

"If the number of plants common to Australia and New Zealand is great, and quite unaccountable for by transport, the absence of certain very extensive groups of the former country is still more incompatible with the theory of extensive migration by oceanic or aerial currents. This absence is most conspicuous in the case of *Eucalypti*, and almost every other genus of *Myrtaceæ*, of the whole immense genus of *Acacia*,

and of its numerous Australian congeners, with the single exception of *Clianthus*, of which there are but two known species, one in Australia, and the other in New Zealand and Norfolk Island.

The rarity of *Proteaceæ*, *Rutaceæ*, and *Stylideæ*, and the absence of *Casuarina* and *Callitris*, of any *Goodeniæ* but *G. littoralis* (equally found in South America), of *Tremandreæ*, *Dilleniaceæ*, and of various genera of *Monocotyledones*, admit of no explanation consistent with migration over water having introduced more than a very few of the plants common to these tracts of land. Considering that *Eucalypti* form the most prevalent forest feature over the greater part of South and East Australia, rivalled by the *Leguminosæ* alone, and that both these Orders (the latter especially) are admirably adapted constitutionally for transport, and that the species are not particularly local or scarce, and grow well wherever sown, the fact of their absence from New Zealand cannot be too strongly pressed on the attention of the botanical geographer; for it is the main cause of the difference between the floras of these two great masses of land being much greater than that between any two equally large contiguous ones on the face of the globe. If no theory of transport will account for these facts, still less will any of variation; for of the three genera of *Leguminosæ* which do inhabit New Zealand, none favor such a theory; one, *Clianthus*, I have just mentioned; the second, *Edwardsia*, consists of one tree, identical with a Juan Fernandez and Chilian one, and unknown in New Holland; and the third genus *(Carmichælia)* is quite peculiar, and consists of a few species feebly allied to some New Holland plants, but exceedingly different in structure from any of that extensive Natural Order."

Dr. Hooker then appends a carefully prepared table of 228 phænogamous species which may be said to represent each other in two or all the three South temperate masses of land, viz., New Zealand (including Auckland and Campbell's Islands), Australia (including Tasmania), and extra-tropical South America, including the Falkland Islands; the list being confined to cases of real and usually very close botanical affinity, to the exclusion of analogical resemblances, however striking. The list is by no means overstrained, nor as full as it might be; since one or two more good instances have occurred to our memory while looking over its columns. On comparing together the Australian and New Zealand columns, we find only fourteen blanks, not filled by known representative species; a similar comparison of the New Zealand and South American columns shows forty-six blanks, sixteen of which are among the Endogens.

On fairly weighing all this testimony, the botanist will perhaps accede to our author's conclusion,—viz., that the floras of these three great areas of land in southern latitudes "exhibit a botanical relationship as strong as that which prevails throughout the lands within the Arctic northern temperate zones: and which is not to be accounted for by any theory of transport or of variation; but which is agreeable to the hypothesis of all being members of a once more extensive flora, which has been broken up by geological and climatic causes." A. G.

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Prof. J. Wyman
with the respects
of the Translator.